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NOTE ON THE DETERMINATION OF SEX IN MAN

By E. T. BREWSTER

Dr John Benjamin Nichols, in a paper recently printed in this journal,¹ sets forth conclusions practically identical with those to which I had come, quite independently, by the same method. He finds, in effect, that in three thousand New England families having six children or more, the actual distribution of sons and daughters is very nearly identical with the theoretical chance distribution. There is nevertheless a slight tendency toward an excess of families in which the children are all of the same sex, and also a somewhat disproportionate number of boys in the largest families. Dr Nichols therefore concludes that sex is entirely independent of environment, but is determined by the dominance of one or the other of the hereditary impulses derived from the two parents. The sex-making forces fight it out in the germ, and in the long run one is about as likely to be prepotent as the other. The slight departures from theory he would explain by the occasional ascendancy of one parent over the other.

Dismissing for the present all questions of interpretation, I shall set forth certain facts in regard to these departures from the theoretical chance distribution.

First of all I turn to the magnitude of the disagreement with chance. Of 1,200 children of known sex, 601 were followed at the next birth by a child of the same sex as themselves, 599 by one of the other sex. This is clearly chance and far within the error of random sampling. Suppose, however, in place of random we take selected cases, a method which in studies of this sort has not been generally employed. In 1,442 cases in which two consecutive children were of the same sex, I find that in 727 a run of two boys or two girls is followed by another of the same sex, and by one of a different sex in 715. Table A shows the results of applying the same method to runs of three, four, five, six, seven, and eight chil-

¹ Vol. VII, No. 1, 1905, pp. 24-36.

dren of like sex in two groups of related families. Throwing out the last case, in which the numbers are too small to have much value, in all cases except one, if two or more consecutive children are of like sex the next stands an appreciably better than average chance of being of that sex also. On the whole this tendency tends to increase with the length of the run. At any rate the final sum, 1,210 of one sort to 1,154 of the other, is significant of the operation of some real cause. Sex, then, is nearly a matter of chance, yet there are special cases in which some sex-determining tendency is also at work, so that a sporting neighbor of the Patriarch Jacob, after the birth of the eleventh consecutive son, might reasonably risk a wager of three to two that number twelve would be a boy also.

TABLE A

SEX OF CHILD AFTER A SUCCESSION OF CHILDREN OF THE SAME SEX

No. Children Alike.	2	3	4	5	6	7	8	Sum.
Family.	Next is Like.	Next is Unlike.						
T***** { like.	314	119	44	13	7	3	1	501
{ dif.	311	117	43	10	4	2	1	488
F***** { like.	413	169	70	35	16	6	0	709
{ dif.	404	173	54	17	11	4	3	666
Sum. { like.	727	288	114	48	23	9	1	1210
{ dif.	715	290	97	27	14	6	4	1154
								2364
Observed ratio like/dif.....	1.02	.99	1.18	1.78	1.64	1.5	.25	1.05
Calculated ratio sexes equal.....	1.00	1.00						1.00
Calculated ratio 11♂ equal 10♀.	1.01+	1.02—	1.02	1.02+	1.0295	1.034	1.038	

I shall now set forth evidence to show that a tendency to depart from the chance distribution of sex in the direction of an excess of boys or girls is correlated with the age of the mother.

My data consist of the published genealogies of eight New England families whose records go back to the early days of the country and come down to the middle of the last century when the number of children to each marriage becomes too small for my purpose. Table B shows the distribution of sex among the first three births and the last three in some fourteen hundred families in which

there were six children or more. From this table it appears that there is a pretty well-marked tendency for mothers to bear boys early in life and girls later. Twenty-three hundred women had among their first three children 3,756 boys and 3,285 girls. The same women had among their last three births 3,594 boys and 3,432 girls. In other words, a group of young women bear 114 boys to each 100 girls: the very same women when along in years bear only 105 boys to each 100 girls. Moreover, taking each of the eight family stocks separately, there is no case in which the number of boys among the last three births exceeds that among the first three. On the other hand, there is but one of the eight in which the number of girls in the first three exceeds that in the last three. Clearly, therefore, there is a correlation between the age of mothers and the sex of the offspring.

TABLE B

SEX OF FIRST THREE AND LAST THREE CHILDREN IN FAMILIES OF SIX OR MORE

Families.	3 Boys.	2 Boys. 1 Girl.	1 Boy. 2 Girls.	3 Girls.	Total Boys.	Total Girls.	Ratio Boys/Girls.	Mean Ratio.
1. F***** { first 3. last 3.	41	128	101	33	480	429	1.12	1.025
	32	118	104	48	436	470	.93	
2. T***** { first 3. last 3.	45	85	85	26	390	333	1.17	1.09
	28	97	88	30	366	363	1.01	
3. R*****.....	53	94	89	31	436	365	1.20	1.09
	35	92	101	35	390	399	.98	
4. W*****.....	37	58	58	17	285	225	1.27	1.16
5. F****.....	30	56	56	26	258	246	1.06	1.05
6. D****.....	19	72	48	22	249	234	1.06	1.045
7. P*****...	20	63	56	20	242	235	1.03	
8. K*****.....	30	114	93	30	411	390	1.06	1.06
9. Sum.....	24	55	90	105	29	411	1.06	
10. Sum calculated if 11 ♂ to 10 ♀ ...	26	48	38	16	220	179	1.23	1.23
	118	334	263	46	13	220	179	
	133	285	302	90	1285	1130	1.14	1.12
	367	940	775	265	1271	1153	1.14	1.10
	346	849	856	289	3756	3285	1.05	1.095
	337	919	836	253	3594	3432		

The correlation need not, however, be direct. In fact the more obvious supposition is that the correlation is primarily with bodily vigor and only incidentally with age. I shall now submit evidence on this point.

Presumably women who bear more than five children are a selected class appreciably more vigorous than the general body of

wives. They should therefore bear a somewhat larger proportion of boys. As a matter of fact, averaging the first three and the last three births, they produce boys and girls in the ratio of 110 to 100; while a random sample of mothers, including this selected class, shows a ratio of only 1.07. This agrees perfectly with the long known fact that there is a preponderance of boys among the first births; and that, according to Dr Nichols, large families contain a disproportionate number of boys and the families of consumptive mothers an excess of girls.

At first sight it would seem easy to test the matter still further by studying the proportion of boys and girls in families of fewer than six children. Unfortunately the apparently small family of a genealogy may be merely one whose members have in part escaped the notice of the compiler; and since the latter is somewhat more likely to overlook girls than boys, the desired ratio is likely to come out higher than it should. Moreover, especially as one comes down toward recent times, families of fewer than six children become practically identical with the general unselected population.

It is, however, possible to select one group of women who are clearly less vigorous of body than their sisters — those, namely, whose husbands marry again. In general the woman whose husband has had children by a second or third wife has died young and is therefore likely to be the sort of woman who should, according to our present theory, bear proportionately fewer boys than average mothers. My numbers are unfortunately small. I find, however, in this class 64 boys and 63 girls; and this, so far as it goes, bears out the theory.

TABLE C

SEX OF CHILDREN OF FIRST, SECOND, AND THIRD WIVES

	First Wife.	Second Wife.	Third Wife.	Second and Third.
♂	64	113	6	119
♀	63	86	2	88
Ratio ♂/♀	1.02	1.31		1.35

There is, moreover, another curious fact (Table C) concerning the children of men who marry more than once. The second wife not only has more children than the first (199 to 127); she also bears a far larger proportion of boys. Second wives, in the families

which I am discussing, have 113 boys and only 86 girls—a ratio of 132 to 100. Third wives have 6 and 2. Now, second wives are presumably, as a whole, somewhat older than first wives; why then do they not bear fewer boys instead of more? The reason I take to be this. The thrifty New Englander, even in his first matrimonial venture, was apt to select his wife with something of an eye to her economic value; how much more then did he at his second attempt choose a woman of vigorous constitution. Men marrying a second time are therefore a selected group which may fairly be assumed to rate bodily stamina somewhat higher as an element in conjugal choice than do less experienced persons. Moreover, the second-wife class may fairly be taken to include an unusual proportion of widows. Now widows, since they are women who have survived their husbands, should be a selected body of unusually vigorous persons. But remarried widows are in addition a selection from this selected group. They are women who have come through the trials of their first venture with enough strength, health, and good looks to render them eligible for a second. First wives and second wives therefore are two special classes—the one more, the other less vigorous than the general run of their contemporaries. Hence the one produces a smaller, the other a larger proportion of boys than the average. All this tends to show that the observed correlation is between sex of offspring and vigor rather than between sex of offspring and age; and this is *a priori* the more probable supposition.

Turning now from the facts to their interpretation, we shall find, I think, that the observed correlation between sex of offspring and age, or vigor, of mothers may be, in part, independent of any initial sex-producing tendency, but is, on the contrary, caused by a higher death rate among fetal and infant boys. There are, for example,¹ 135–140 still-born boys to each 100 girls. Moreover it appears from the records of one Dublin hospital that 1 girl and 16 boys died within half an hour of birth; at the end of the first hour the numbers had become 2 and 19; and after six hours, 7 and 29. It is well known that while boys outnumber girls at birth, occasionally by as many as 130 to 100, they tend to die off so much faster

¹ Havelock Ellis, *Man and Woman*, p. 432.

that they usually soon drop behind in absolute numbers. It is not known, apparently, what the comparative death-rate of the two sexes is among young embryos. If then, given an initial excess of boys, we suppose that boys, since they are larger than girls, are more tax upon their mothers so that the older and less vigorous women lose more of them, while the younger and more vigorous mothers lose fewer, the observed distribution of sex in families is, in part, accounted for.

Nevertheless, this explanation alone is apparently not sufficient. The change in the ratio of boys to girls from 114 to 105, as shown in Table B, is more than can reasonably be attributed to this cause alone. Moreover, the ninth and tenth lines of Table B show that in both groups of births the number of unmixed families, both of boys and of girls, is in excess of the number due to chance. This could hardly be the case if the cause involved were one which operates on the boys alone. Nor is this explanation sufficient to account entirely for the runs in Table A. Notwithstanding this, if we knew, as we do not, anything about the reasons why ova and very young embryos fail to give rise to living infants, it might very well appear that the two sexes are in this respect, as in most others, by no means on a level. Causes of the same general order as those which take off more boys in infancy, more women in early adult life, and more men in old age, which destroy more women by phthisis and more men by apoplexy, might conceivably operate from the beginning and, the sexes being originally equal in numbers, cause just the distribution which actually appears.

After all, the significant thing about the distribution of sex is that it is so nearly in accord with chance. Only by supposing a chance distribution somewhat modified by some variable cause acting within small limits can we avoid the difficulties inherent in all theories which involve the idea of "prepotency," and assign to the same cause the general law and the departures from it.

Summary

1. Sex in man is nearly but not quite a matter of chance.
2. In large families and among the first three births in families of six children or more the proportion of boys is more than average.

3. This is probably due to the greater vigor of the mothers.
4. This excess of boys is not entirely due to "prepotency" but at least in part to the fact that these mothers lose fewer boys than average women.
5. The same principle might conceivably be extended to explain all departures from chance distribution.

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